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the true value. The weight of the result given by these observations is certainly very great.

I know of only one cause which has yet been assigned as a possible explanation of the large value given by these meridian observations—viz. chromatic dispersion by the atmosphere. There is, however, no difficulty in proving mathematically that the illumination of the upper limb of a planet is increased and not decreased relatively to the lower limb from this cause. This will be clear enough if anyone will merely consider that the red light from the lower limb cannot be strengthened by the yellow, green, and blue lights of greater refrangibility, whilst the red light from the upper limb will be supplemented by light of greater refrangibility from portions of the planet at slightly greater zenith distances. Under any conditions of illumination, therefore, this cause cannot make the observer cut deeper into the upper limb than into the lower. Atmospheric dispersion might, therefore, be appealed to with some slight plausibility as a cause of the small value which has been deduced from the Heliometer measures, but most certainly it is not directly the cause of the large value found from the meridian observations.

Note on Mr. Neison's Paper on the Corrections to Adams's Semi-Diameter of the Moon. By E. J. Stone, M.A., F.R.S.

I have felt great reluctance to continue any longer in our *Notices* a discussion upon these questions of the diameter of the Moon. The question is not, however, simply whether I am right, or Mr. Neison right, upon a point of combined mathematics and arithmetic, but whether the parallactic inequality deduced from a discussion of a valuable series of observations affords, or does not afford, evidence in favour of a certain supposed long inequality in the Moon's longitude. If that long inequality does exist with a coefficient $1''\cdot2$ it is of the utmost importance that its existence should be recognised, and that its changes should be allowed for in our Lunar Theory; but if the inequality does not exist with any such coefficient, it is of even greater importance that its variations should not be allowed to vitiate our results. I have, since the last meeting, re-examined my work, and I find it correct. But before the publication of Mr. Neison's separate results I was unable to offer any possible explanation of the systematic discordances between the results obtained by us. The error into which Mr. Neison has fallen is now, however, clear to me.

The following are the formulæ from which my results have been obtained:—

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Let

B = Burckhardt's parallax in seconds of arc.

δA = The correction to B found directly from Prof. Adams's formulæ.

S = Tabular semi-diameter adopted in any year.

x = The mean correction required by Adams's semi-diameter with the factor 0·273114.

Let

$$\frac{\text{Observed diameter} - \text{Tabular diameter}}{\text{Tabular diameter.}} = n.$$

Then

$$n + 1 = \frac{(B + \delta A)\, 0{\cdot}273114 + x}{S},$$

or

$$x = \left(n + 1 - \frac{B}{S} 0{\cdot}273114\right) S - \delta A \,.\, 0{\cdot}273114.$$

Now, in

1853, 1854, 1855,

$$S = B\, 0{\cdot}2725 \left(1 + \frac{1}{360}\right);$$

$$x = \left\{ n + 1 - \frac{0{\cdot}273114}{0{\cdot}2725\left(1 + \frac{1}{360}\right)} \right\} S - \delta A\; 0{\cdot}273114,$$

$$x = (n + 0{\cdot}0005233)\, S - \delta A\; 0{\cdot}273114.$$

If we refer to the "Tables containing corrections to be applied to the values of the Moon's equatoreal horizontal parallax given in the *Nautical Almanac* 1840–1856, in order to make them agree with those calculated from Mr. Adams's tables" (*Appendix N.A.* 1856), we can extract numbers which will give us δA. But, on reference to the *Nautical Almanac* 1853, 1854, and 1855, it will be found that the *N.A.* Parallax = B $(1 + \frac{1}{1200})$.

It follows, therefore, that if δN is the correction found in the *Appendix N.A.* 1856,

$$B + \delta A = B\left(1 + \frac{1}{1200}\right) + \delta N,$$

or

$$\delta A = \frac{B}{1200} + \delta N;$$

therefore

$$x = (n + 0{\cdot}0005233)\, S - \delta N \,.\, 0{\cdot}273114 - \frac{B}{1200}\, 0{\cdot}273114.$$

Now, if we neglect the term $\frac{B}{1200}\, 0{\cdot}273114$, or assume that the corrections given in the *Appendix N.A.* 1856, are corrections

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to Burckhardt's Parallax, and not to the *N.A.* Parallax, we shall substantially obtain Mr. Neison's results; but these results are clearly wrong to an extent

$$=\frac{B}{1200}\,0{\cdot}273114=\frac{3420}{1200}\times 0{\cdot}273114=0''{\cdot}78$$

very approximately.

To show that the corrections given in the *Appendix N.A.* 1856, are corrections to the adopted parallaxes of the *N.A.*, and not to Burckhardt's tables, it is sufficient to extract the following corrections:—

1852	Dec.	$30 = +2''{\cdot}8$	Corrections to Burckhardt
1852		$31 = +2{\cdot}6$	
1853	Jan.	$1 = -0{\cdot}4$	Corrections to Burckhardt $(1+\frac{1}{1200})$
1853		$2 = -0{\cdot}3$	

If we apply the correction $0''{\cdot}78$ to Mr. Neison's results, and compare them with mine given in the *Notices* for December 1881, we obtain as follows. I have changed the signs, so that the quantities given in these tables are the *corrections* to be applied to Adams's semi-diameter to make it agree, in the mean, with the observed horizontal semi-diameters:—

	Neison's corrections to Adams'.			Stone's corrections to Adams'	Neison's results corrected.
	Obs.	″	Obs.	″	″
1851	6	−0·21	7	−0·43	−0·21
1852	3	+0·86	3	+0·71	+0·86
1853	4	+1·73	4	+0·97	+0·95
1854	3	−0·39	3	−1·17	−1·17
1855	3	−0·25	3	−0·94	−1·03
1856	3	−1·75	3	−1·77	−1·75
1857	3	−0·76	3	−0·76	−0·76
1858	5	−1·02	5	−1·02	−1·02

I have no doubt about the substantial accuracy of my results, but the discordances between the outstanding results are now unimportant. These observations make Adams's semi-diameter too large by $0''{\cdot}51$, but Messrs. Campbell and Neison have assumed, in their work, that this semi-diameter is too small by $0''{\cdot}18$ for the same group of observations. The rejection by Mr. Neison of the diameters observed by the computers at Greenwich, whilst their observations at Quadratures are retained, is certainly unsatisfactory. With respect to the second point, I have nothing more to remark than that my work is accurate, and that the corrections to the diameter deduced by me refer to the period 1843–1851 under discussion; and the results certainly do not